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# Claude Bernard and the Internal Environment

A MEMORIAL SYMPOSIUM

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## General Discussion

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Chairperson JOSHUA LEDERBERG

**Prof. Lederberg:** Thank you, Gene. I accept your invitation to attend the renewal of this symposium a hundred years hence.

I'd like to make a few provocative remarks just to raise the temperature a little bit and see which ventricle gets the benefit of it. Just as a preface to what Dr. Robin indicated, the notion that short-range adaptations are going to be free of biological cost may be overlooking the evolutionary price that has to be paid for doing that. The constraint on a system that requires that it be able to respond in matters of seconds or minutes to changes in the immediate milieu cannot help but affect the efficiency of those systems with respect to other parameters. We can't have indefinite flexibility without paying some price in terms of specialized capability as well. We have no way of checking that out. We don't have alternative worlds of organisms evolved in different ways in order to make that comparison. So, evolutionary costs are difficult to estimate.

I quite intentionally introduced the conception of evolution into this discussion. I was quite intrigued by Jean Bernard's image of the colloquium that might have included Claude Bernard, Louis Pasteur, Charles Darwin, and Gregor Mendel. We know that Mendel was, in fact, for various reasons, sometimes exaggerated, not part of the main current of scientific thought for 35 years after his work was first published. The other three that I've mentioned surely were. I put it as a question of historical naiveté, on my own part, exactly what were the interactions between these figures? I have not really examined Bernard to see whether he quotes Darwin at any point. I do not recall Darwin having quoted Bernard in any way. I have been personally very interested in the lack of interweaving of the intellectual strands of Darwin and Pasteur, and, as far as I have been able to determine, they might almost not have existed in one another's milieu, but perhaps that can be illuminated further. I just put that out as a question in the factual intellectual history of that time about those relationships.

I do not, in the present discussion, see nearly as much emphasis on the evolutionary framework of Bernard's thinking as I have heard about his embryological perceptions. Again, I just don't know. As a matter of fact, the extent to which he had incorporated that—the notion, for example, that the internal environment can be, in a genetic sense, traced to the oceans, which is, I believe, a notion that Darwin, in fact, did have fairly strongly. Did he correlate the plasma with the early sea water, which was presumably the origin of the environment of cellular functions? And it's a fairly common perception, at least since Henderson.

I would just like to make one further remark on the tension between system and experiment that erupted at various points in the discussion, and the statement of apparent contradictions in Bernard's career that were brought out, particularly by Professor Fruton and by others. I certainly do not wish to pose as a professional student of Bernard myself; my remarks were entirely a reflection of what I heard during the actual discourse and are not based on any original insight into what Bernard, himself, has to say. But I did describe his system and the impact that this has had on elegant and pedagogically effective statements of scientific conclusions as on the one hand being very much part of the tradition of scientific exposition since his time, and I believe it was corroborated that historically it may have had exactly that role. On the other hand, I described that as a falsification of which we are all guilty. That is to say that, in our own exposition of our scientific reports, we do not make anecdotal statements; we do not make historically correct statements; we generally do not report the experiments that didn't work; we often discard experiments that we had reason to believe had extraneous variables, and don't even mention them, although it is not always easy to substantiate whether they were legitimately part of the framework of experience or not. We are content in our publications to elect merely with recipes for replication of the results. I think that's the one categorical requirement of scientific publication. That has many virtues, many advantages. In fact, it was suggested that the kind of pedagogical elegance that may also contribute may outweigh the importance of dragging in all the dirty linen that led to all these very fine fabrics that are eventually produced. In very large measure I would agree, but there is still, besides the question of historical validity, an important policy error that flows from the overelegant presentation of scientific results. That has to do with the very widely held perception, and even self-delusion on the part of our own scientific colleagues, about the extent to which scientific discovery can be programmed in advance. Now, with all of us complaining, with very few exceptions, about that happening from a bureaucracy in Washington, that the difficulties of managing crusades against this, that, or the other disease, in terms of generalized programming of discovery, we do not hear so much about what I would hold to be a more pervasive fallacy that operates in the administration of the gate-keeping systems at the present time,

and particularly in matters like the reviews of grant applications. The demand for the meticulous prediction in advance of which experiments will be done tomorrow, the outlining of the details of protocols which are required today as part and parcel of the peer review process, of which we are all part, and of which we are all culprits, is certainly within the model of the great systems and methods of the nineteenth century, but flies in the face of the reality of scientific discovery, which is full of the false starts, the serendipity, the absolute unpredictability of any really important discovery or any really important consequence. To that extent, I think the persistence with this model of representation of scientific discovery as the neat packages in which they appear when they are finally published, indeed, does have a highly pernicious effect in the selection against creativity in the present framework of the granting system. So, I think there may be more to be said on both sides.

However, system is not to be totally deprived. We cannot be pure empiricists; the people who insist on that the most are simply asserting their own philosophical system in their own framework of examination.

Much was made of Bernard's skill in observation, that he would understand when there were anomalies, discrepancies, peculiarities of events that escaped his colleagues. That's only possible if there is, indeed, a pervasive system outlook on the part of the investigator. It may not be the same philosophy that he wrote down in his books later on, but he must have had one. And so must any acute observer of nature, any natural historian, any skilled experimentalist. Well, with those provocations, I open this for a general discussion.

I'd certainly be particularly eager to have some factual responses to my questions on the intellectual history of the latter part of the nineteenth century.

**Prof. Robin:** I don't know how factual my comments are. I think most reviews of Bernard's life indicate that he did not take evolution very seriously (one was suggesting that the reason he didn't was that, even if the concept was valid, not much could be done about it). He apparently didn't know much about sweet peas, let alone fruit flies. There are certainly people better acquainted with his views who might wish to comment on a possible connection with Mendelian genetics.

The relationship between Pasteur and Bernard is, of course, well known. Pasteur maintained that living yeast was required for fermentation. Bernard advocated the view that cell-free systems (in modern terms) might be able to glycolyze. Unfortunately, Bernard may have thrown out the germ theory of disease during the course of the argument. They had a famous scientific squabble. However, they respected each other deeply, and we have already heard Pasteur's formal tribute to Bernard. Bernard was deeply moved by Pasteur's tribute. It is also recorded that Pasteur was largely responsible for the support which Napoleon III ultimately provided to Bernard's work.